

& Department of
Medical Genetics

Samuel
April 7, 1957

Dear Tracy:

Esther and I have just been away for a month in France and Britain, and had of course a wonderful time. Av' Mitchison will be visiting you later this week; I hope he will have arranged with you to be able to squeeze in a visit to Madison, for very good reasons besides social and scientific causerie. (I don't want to pretend subtlety: if Av's really contemplating leaving Edinburgh at all, Wisconsin would have an excellent niche for him (I hope) either in or near Medical Genetics.

Our own plans have been more complicated, but are somewhat simpler now. I don't know how au courant you've been lately in Harvard Biology, but I had a very substantial offer from them just before we took off for Europe. This one had been cleared with the President. However, Esther and I have decided not to pursue it, and I've written Thimann accordingly. Ditto Stanford. This leaves Berkeley (still actively unsettled) and Madison. Here, the formal organization of Medical Genetics has gone through, and I ought to emphasize that this was developing independently of external stimuli. Medical Genetics is quite integral (by means of joint appointments) with Genetics, and it is even conceivable that we might eventually regroup to form a Genetics-Science department. At present, however, Genetics has strong applied commitments, e.g., potato, fur animal, cattle and swine, forest-tree, improvement, in addition to the useless things such as I do, and it would be difficult for one chairman to give proper leadership for these matters at the same time as organize a genetic program in the medical faculty, not to mention the problems of budget-splitting in two colleges as the present administration goes. I say all this just to scotch the idea that there is anything more than an administrative basis for the separation, as far as it goes.

There aren't many precedents for Medical Genetics, viz. genetics in a medical school, not just human genetics, and we have to do a lot of thinking about our detailed program. It may help us along to have a symposium on the subject, which is in fact now ^{being} organized for next April. I hope one way or another you'll be interested to come: the program is just beginning to be worked out now.

In the lab., most of my own time still goes to protoplasts and their proliferation (by blebs) to give L colonies, and the like. My initial hope that these would lead to a successful dna-transduction in E. coli remains still unsatisfied, but we haven't given this a maximum effort. Iino and I are still concerned about the determinism of phase-variation. Until now, we have mainly gone after the genetics of monophasic variants, but lately he has started some trials on environmental effects. There may be something in temperature shocks, but the effects are too small so far to exclude even a minute differential in killing rate.

Yours,

Joshua Lederberg